paths ending in North Africa. The fourth (D), fifth (E), and sixth (F) main routes depart from the extreme north of Siberia. The fourth (D) ascending the river Ob, branches out near Tobolsk—one track, diverging to the Volga, descends that river and so passes to the Sea of Azov, the Black Sea, and thence by the Bosphorus and Ægean, to Egypt; another track makes for the Caspian by way of the Ural River and so leads to the Persian Gulf, while two more are lost sight of on the steppes. The fifth (E) mounts the Jennesei to Lake Baikal and so passes into Mongolia. The sixth (F) ascends the Lena and striking the Upper Amoor reaches the Sea of Japan, where it coalesces with the seventh (G) and eighth (O) which run from the eastern portion of Siberia and Kamchátka. Besides these the ninth (X) starting from Greenland and Iceland, passes by the Færoes to the British Islands and so joining the second (B) and third (C), runs down the French coast."

All these routes are plainly laid down on the map which accompanies the work, and in the absence of more precise information, it will hardly be in the power of any British ornithologist to dispute them, though, as before stated, we must hold them to be in a great measure conjectural. In the following chapters the author shows how necessary it is to know the principal routes taken by birds in their migrations before we can understand or reason intelligibly on their movements, and of very great interest are his remarks on the Genetic Import of Regular and Irregular Lines of Travel, and on the So-called Migratory Instinct (chaps. ix. and x.), greatly amplified in the German version from the brief paragraphs which represent them in the Swedish original. They are, however, it must be confessed, somewhat verbose; but, for all that, they are well worth reading. Though Herr Palmén refers to an article which appeared in these columns some years ago (NATURE, vol. x. p. 415), he does not seem to be aware of the theory subsequently propounded by Mr. Wallace (vol. x. p. 459) as to the possible or probable origin of migratory habits, wherein is expressed, in far fewer words than his own, what appears to be essentially the same thing. For "Migratory Instinct" Herr Palmén substitutes "Experience" as the piloting power, and though there is much to be said in favour of this explanation in many cases, others there are in which it seems to break down utterly. How do the young cuckoos which stay in this country a month or six weeks after their parents (whom, let us remember, they have never known) have departed find their way to Africa? And how do the scores, hundreds, or thousands of rapacious and wading birds, whose elders do not accompany them, manage in their autumnal journeys to arrive more or less punctually at the spot which countless generations of their predecessors have reached before them? They have had no "experience," and though doubtless many perish by the way, a very large proportion year after year hit off exactly, and at the first intention, the ancestral landing-place. What, also, can "experience," which, after all, means only a knowledge of landmarks, do for the species which travel by night, as seems to be the habit of very many birds, or for those which, like at least two of the annual visitants to New Zealand, traverse a waste of waters? At present no solution of the mystery offers itself, at present such knowledge may be too wonderful for us; but, high as it is, our faith in the progress of science forbids us to say that we cannot attain unto it.

OUR BOOK SHELF

Dynamics; or, Theoretical Mechanics, in Accordance with the Syllabus of the Science and Art Department. By J. T. Bottomley, M.A., F.R.S.E., F.C.S. (London and Glasgow: William Collins, Sons, and Co., 1877.)

THIS little text-book is issued by Messrs. Collins as one of their Elementary Science Series, and will prove useful to beginners, by rendering them familiar, at an early stage of their studies, with the more precise definitions and nomenclature which have been introduced by modern writers on dynamics. The distinction, for instance, between the centre of gravity and the centre of inertia is much more clearly pointed out than is usual in elementary works, and the statement that "there is only a limited number of classes of bodies that possess a centre of gravity" will probably be read by many with surprise. The measurement, composition, and resolution of velocities are treated of in the chapter preceding that on force, and the methods of measuring forces in terms either of gravitation units or absolute units are well and fully discussed. The definition of work given in the last chapter might, we think, be amended. As it stands at present it might lead the student to suppose that no work is done by an agent moving a body, unless the motion is created in opposition to a resisting force, though the language employed in some of the examples would be sufficient to correct such a supposition. Throughout the work the author assists the student to obtain "clear physical conceptions regarding the first principles of dynamics," by frequently directing his attention to the experimental proofs of the various laws he enunciates, and by hinting at the physical, rather than the mathematical, developments of his subject.

On these grounds, we have formed a very favourable opinion of Mr. Bottomley's work, and we have no doubt that it will meet with the success it deserves among a wider class of students than that for which it is specially designed. A. R.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Evolution and the Vegetable Kingdom

MR. CARRUTHERS has embodied in the *Contemporary Review* the substance of his Presidential addresses to the Geologists' Association, on which we would offer a few points for consideration.

Although not agreeing with Mr. Carruthers as to the inferences to be drawn from the present state of our knowledge of fossil vegetable remains, we cannot but admire the earnestness with which he makes a stand in what we regard as a losing cause. We set a high value on his researches in fossil botany, and his work is characterised by unvarying and careful exactitude. Whatever may be his theories, his reputation will rest on a solid basis of work. Palæontologists have to thank him for unvarying kindness and readiness to aid them in their researches, forming a marked exception to the treatment which botanists usually give the subject.

In discussing this question, we must keep well in mind the teaching of Sir Charles Lyell, first as to the insufficiency of the geological record, especially with regard to land-surfaces. Considering the denudation and the wasting action of the waves to which remnants of terrestrial conditions are exposed during the slow process of their submergence beneath the sea, and again during their gradual upheaval, it is surprising to us not that so few records are preserved, but that any vestiges whatever remain. Secondly, with regard to lapse of time, we must get the "chill of poverty out of our bones," and not misinterpret "the sign of successive events, and conclude that thousands of years were implied where the language of nature imports millions." Mr. Carruthers admits the imperfection of the geological record, although scarcely with sufficient emphasis, and compares its fragmentary condition to a tablet containing

the remains of an unknown inscription represented by only a few of its numerous letters, each of which occupies its proper relative position to the known and unknown letters of the inscription. This is hardly a happy simile as the relative ages of the beds and strata containing vegetable remains, scattered over the world, are certainly very far from settled, and their correlative sequence is in numerous instances still the subject of great discussion. The relative position of these letters is therefore at present but vaguely known.

Haeckel supposes "that the sub-marine forests of the primordial period were formed by the huge brown algæ or fucoidæ." In the 70,000 feet of sedimentary rocks, from the Laurentian to the Devonian, beds of carbon and graphite are abundant; the only known vegetation throughout that period is of algæ. So far only Mr. Carruthers agrees with Haeckel; from this point his views diverge. During the period of the deposition of these 70,000 feet, time enough surely elapsed (if time only be required) for the evolution of vascular cryptogams from algæ. In the Silurian had vegetation been equally fitted to resist decay, we should probably have had plant evolution indicated side by side with that of animals. Before leaving the subject of algæ, we must differ from Mr. Carruthers, who says that if rich floras had existed, the limestones of the Llandovery rocks at Malvern would have preserved them. In these marine rocks at the most sea-weeds could be expected, and limestone of whatever age do not usually preserve such traces; but we know that floras existed by the carbon and graphite before-mentioned.

Mr. Carruthers urges what he considers as fatal objections to the doctrine of evolution; his arguments may be briefly stated as follows:—

1. The simultaneous appearance of the three principal groups of vascular cryptogams, even in a more highly organised condition than their living representatives.

2. The early appearance of gymnosperms and the want of connecting links between these and the lycopods from which they are supposed to have been developed.

3. The early appearance of monocotyledons.

4. The sudden appearance of dicotyledons, not only in the lower form as Apetalæ, but also as Dialypetalæ and Gamopetalæ.

5. The persistence in specific character of *Salix polaris* from the glacial period until now and over a wide range.

Let us now see on what facts these objections are founded, and whether the facts are not open to other interpretations.

1. The evolution theory requires that lower groups have developed until the amount of organisation was reached required to enable them to fulfil the conditions under which they live, and to occupy vacant ground in the economy of nature. This required amount of development may be more or less quickly attained, and the development of the organism then remains almost stationary. Side by side with this development, other development goes on unceasingly, leading to the gradual evolution of entirely different and more highly organised forms. The cryptogams are paralleled amongst vertebrates by the early and specialised development of reptilia; amongst crustacea by trilobites, &c. In like way the tetrabranchiate cephalopods, the brachiopods, and numerous other mollusca, whose hard shells resisting decay, have enabled us to trace their life history, have come down to the present time, just as these vascular cryptogams have, not in their most complex and differentiated forms. We do not expect to find sustained progressive development in the lower animal and vegetable groups, and are not surprised at evidence of actual reversion.

2. On their first appearance "the Gymnosperms do not," Mr. Carruthers says, "present a generalised type, but a remarkable variety of genera and species, all as highly differentiated as any of the existing forms." Now if this is absolutely the case, and their *first appearance* in life is coincident with their first appearance in the fossil record, there is no doubt that they were specially created, and there is no need of further argument. But the point not yet proved is that the two are coincident. The occurrence of coniferæ in the Devonian is only known by wood with coniferous structure. The fruit and foliage, if known, might possibly afford an indication of the mode in which their course of evolution, as suggested by Haeckel, had taken place. Unger has described anomalous woods from Thuringian rocks of Devonian age. "Had these been of earlier age than Miller's Cromarty wood they might have been looked upon as one of the steps leading up to the true Coniferous structure." These may yet be looked on as steps, for the relative age of these Devonian rocks is still to be fixed. This occurrence of anomalous wood is at all events not to be overlooked, if, as is stated, the gymnosperms both structurally and embryologically form the transition group from ferns to angiosperms. The occurrence of Coniferous wood in Devonian rocks rather shows how great are the gaps to be filled up, and that the evolution of the gymnosperms commenced at an earlier period than was supposed, during the formation of the great carbon layers of the older rocks, and side by side with the development of ferns and lycopods. The common ancestors of the spore-producing lycopod and the seed-bearing gymnosperm are to be sought in remoter times even than the Devonian. There is no evidence that the Devonian woods were those of the higher and diœcious conifers and that conifers first appeared in this form. Little is really known of the earlier coniferæ, but the cycads—the lowest form, and most nearly allied to ferns—were far more abundant formerly than at present. Schimper writes as follows:—"What form the prototype of our conifers took in carboniferous times is not satisfactorily settled, neither fruit nor foliage having been discovered which could be placed in any order with certainty. The few fragments placed in Abietinæ may belong to Lepidodendron."[1] In the Permian rocks conifers are abundant.

3. Concerning the appearance of Monocotyledons at the base of the Trias, the first true monocotyledon, Mr. Carruthers states, is the stem and spike of an aroïdeous plant from the lowest carboniferous strata near Edinburgh.[2] Recently a number of additional specimens have come to light, but Mr. Etheridge, junior, who is referred to by Mr. Carruthers as having found them, does not we believe regard them to be monocotyledons at all; and in this view, although we have not examined them, we are inclined to concur, because it seems unlikely that so many spikes should be found without foliage. In the *Transactions* of the Botanical Society of Edinburgh, vol. xii. p. 152, Mr. Etheridge points out that the stem of Pothocites was branched, and what was thought by Paterson to be the remains of a deciduous spathe was one of a series of small enlargements which "occur along the course of the stem at regular intervals, jutting out one on each side opposite one another." This additional information throws still greater doubt on the correctness of the determination. At Bournemouth, where aroids are abundant, leaves only have been found without a single spike. This is not the first time monocotyledons have been supposed to be present in the Carboniferous; for example, Cordaites, a plant now acknowledged to be a gymnosperm—but whether a cycad or conifer is still, according to Schimper, a matter of doubt—was formerly supposed to be a palm, and subsequently a Yucca or Dracæna. The curious twisted bodies called Spirangium are assumed to be monocotyledons on very slender grounds, their affinities, according to high authorities, being completely unknown. Nevertheless we find Mr. Carruthers, referring to the Carboniferous, says:—"Including these fruits there are probably eight species of monocotyledons in the later Palæozoic rocks." But *excluding* them there is but one, and that, as just shown, of an extremely doubtful nature. Monocotyledons occur doubtfully in the Trias as Yuccites, and in many forms in the Lias, agreeing so far with Haeckel's table of their pedigree. They gradually increase in number until the present day. Although we question the reality, we think the early appearance of a monocotyledon, even if it had occurred, would no more invalidate the theory of evolution than does the equally unlooked-for occurrence of mammalian remains in secondary rocks, invalidate the theory in reference to animal remains.

4. The next point Mr. Carruthers brings before us is the appearance of dicotyledons, and as their testimony for or against evolution is very important, this testimony deserves examination at some length. Mr. Carruthers regards, as the most fatal objection to the evolution theory, the supposed fact that representatives of all the three great groups appear simultaneously in the Upper Cretaceous rocks. Dicotyledons have been found as low down as the Neocomian, and their discovery in rocks of this age is quite recent. Still the evidence that this is their earliest appearance is purely negative, and no hypothesis is satisfactory which is based entirely on negative evidence. It is probable that dicotyledons may be found in yet earlier rocks—perhaps quite early, although playing an extremely subordinate part. The Wealden has yielded no monocotyledons, yet we know that they must have existed; may not then the earlier forms of dicotyledons also have existed? We may parallel the case of the mammals from the Purbecks. The Purbeck fauna was considered to show no trace of mammals until the examination of a particular

[1] *Pinus anthracina*, Lindley and Hutton, is "certainly a fragment of a Lepidodendroid fruit."—Carr., *Geol. Mag.*, vol. ix. p. 58.

[2] *Pothocites grantoni*, Paterson. *Trans. Bot. Soc. Edinb.*, vol. i. p. 45, pl. 3, f. 1-3. Not mentioned by Haeckel or Schimper.

small area of the Purbeck beds revealed their presence in numbers; had this spot not been quarried it would have been supposed for years that mammalia had made their appearance in Eocene times. Some causes, tending to make the preservation of dicotyledons difficult were discussed in NATURE, vol. xv. p. 281, and need not be further alluded to here. Mere localised patches of plant remains are not an unerring index of the character of a flora at any period. At Bournemouth there are patches just underlying the lowest marine beds, which are crowded with ferns only; other patches contain nothing but ferns, aroids, and gymnosperms. Had these patches been isolated, inferences of a most misleading character would have been drawn.

The upper Cretaceous floras are known to us principally from Aix-la-Chapelle and from America; but as in both these—indeed in most cases—the supposed Cretaceous beds containing plant remains rest on palæozoic rocks, their relative age is a matter of uncertainty. M. Barrois fixes it as contemporaneous with his zone of Belemnitella, but whether he is right in this supposition or not, the flora contains ferns and other plants which seem identical with those of the Bournemouth beds. In America, in the Dakota group, we have leaf beds 400 feet thick of the supposed age of our gray chalk, but the associated marine beds have, mixed with decidedly Cretaceous forms, shells approaching very closely those of our London clay. It seems more logical to determine the age of a rock by the incoming of new types than by the lingering of old, and the whole palæontological evidence shows that these beds are at most intermediate in age between our Eocene and Chalk, the enormous gap between which is probably filled up here by some 2,200 feet of strata. American geologists are not agreed as to their age. It would be out of place to discuss this subject at length, but enough is said to show that the relative ages of these floras is not definitely known, and that no series of arguments based on their relative sequence is, at present, entitled to any weight. M. Lesquereux finds evidence in support of evolution in the flora of Dakota, "in the remarkable disproportion of genera compared to species;" and in the sameness of the leaves, which are "mostly entire, coarsely veined, and coriaceous, the difficulty of separating them into distinct groups, by fixed characters, the numerous forms of leaf which, seen separately, represent different species, or even genera, and which, considered in series or groups, appear undividable into sections." When, however, he theorises, we see that he makes use of the same arguments against evolution as those put forward by Mr. Carruthers. Von Ettingshausen, on the other hand, who has paid much attention to the subject, states that he is able to trace the ancestry of our present floras back to simple elements in Tertiary times, and these to still simpler and more united types in Cretaceous times. In his works a number of examples are given. The flora of Sezanne, whose age as Lower Eocene may be accepted, is closely analagous with that of Bournemouth.

Now let us examine the manner in which determinations of fossil leaves from these earlier rocks have been made, and see whether they are sufficiently reliable to entitle us to form any theories whatever as to the simultaneous appearance of the three divisions of Dicotyledons. Let us take the flora of Dakota.

Of *Polypetalæ* we have Liriodendron, founded on two fragments, and Magnolia on two fragments. These fragments are of simple leaves and possess no character whatever in themselves, upon which they can be determined. Magnolia, for instance, is determined from the similarity in form to leaves described as Magnolia by Heer in the Flora of Greenland, which themselves are supposed to be Magnolia because they resemble (not specifically) Magnolias from the Miocene of Europe. In Menespermites, the third genus, the name indicates that its affinities are vague, and we accordingly see that it had been formerly described as Dombeyopsis, Acer, Populites. The *Gamopetalæ* are represented by three genera. Of these Andromeda is determined on two fragments and one indistinct leaf of simple lanceolate form; Diospyros, formerly described as Quercus, is determined from one simple and ovate leaf resembling Laurus, the other a round and simple leaf; while Brumelia is still more unsatisfactory, and has been previously thought to be either Laurus or Quercus. The determinations have been changed, as we see by the position of the plates and the figures on the plates, many times during the progress of the work, and it is not too much to say that all the determinations of leaves of Polypetalæ and Gamopetalæ from this flora are vague and unsatisfactory, and no one would be more ready to acknowledge this than Mr. Carruthers himself. We do not find fault so much with the determinations themselves, which are probably the best that could be made from such material, but we think it premature to base any theories upon them as to the simultaneous appearance with the Apetalæ of the more highly organised Dicotyledons.

In the Eocene and Miocene we have, however, richer materials, and the variety and completeness of the fossil flora become conspicuous; the forms, as Lyell says, "were perfect, changing, but always becoming more and more like, generically and specifically, to those now living." Von Ettingshausen has traced the direct descent of many living species back to the Miocene, sometimes two or more species to a common parent stock.

5. With regard to the persistence of *Salix polaris*, it appears to be simply a case of a plant becoming thoroughly adapted to certain conditions of life which were met with in England during the glacial period, and are present now in extreme northern regions. Why *Salix polaris* should have varied since glacial times more than mollusca and other animal life is not apparent. The intermediate forms which should connect willows and poplars have not been found, but as poplar-like leaves have been met with in lower cretaceous rocks, it is probable that the order of Salicaceæ is an extremely ancient one, and the single generalised form must be sought for in remoter times even than the Cretaceous.

Our general broad knowledge of the succession of plant life, as testified by the rocks, is too well known to need recapitulating here. Schimper enters in detail into its history. In the Silurian, Algæ; in the Devonian ferns and Lycopods, reaching their apogee of development in the Carboniferous; and in the Permian the conifers first take an important position. The Triassic indicates a great gap, and may be considered the reign of gymnosperms, whilst the incoming of the phanerogams is placed beyond doubt. The Jurassic presents another hiatus, and but little is known of its flora.[1] Heer, however, infers, from the entomological fauna, that there were no leafy trees in the Lias. The oolitic rocks contain abundance of cycads. The Wealden and Neocomian vegetation has left us little more than gymnosperms and ferns. With the upper cretaceous period dicotyledons are abundant, but their incoming is traced to older rocks. The Eocene contains rich assemblages of dicotyledons, principally apetalous, and the Miocene, better known, a still greater variety. We see the same plan of development in the individual; and, as Prof. Huxley recently stated in a lecture at South Kensington, "we can trace living plants from the most gigantic and complicated tree, step by step down through many gradations to the lowest algæ, the lichens, and on down to a piece of animal jelly."

Thus we find on reviewing the evidence that has been brought forward, that other interpretations may be put upon the facts presented to us by Mr. Carruthers. J. S. G.

IN an article in this month's *Contemporary*, entitled "Evolution and the Vegetable Kingdom," Mr. Carruthers refers incidentally to a question that deserves the careful consideration of all who accept the doctrine of evolution; viz., whether the earliest type of flower was hermaphrodite or unisexual. Alluding to the abundance and variety of palæozoic gymnosperms, as evidenced by the numerous fruits that have been discovered in the carboniferous measures, he lays stress on the fact that "they all belong to the Taxineous group of conifers . . . . that the plants of this section are all diœcious, *i.e.* having the sexes on different plants. If the occurrence of the germ and sperm elements in different organs, and even in different individuals, is evidence, as it is held, of higher development in phanerogams, then it is important to notice the order of appearance of diœcious and monœcious groups in relation to those with hermaphrodite flowers. Advocates of evolution hold that dimorphic plants are now in a transition stage progressing towards a diœcious condition. The conifers attained to the highest known development as regards this element of their structure on their first appearance."

If Mr. Darwin be regarded as an exponent of the views held by "advocates of evolution," we find that he expresses himself very differently. From the following passages in his recently published work on "The Effects of Cross and Self-fertilisation in the Vegetable Kingdom," he would seem to consider the primordial condition to be unisexual. "There is good reason to believe that the first plants which appeared on this earth were cryptogamic. . . . . As soon as plants became phanerogamic and grew on the dry ground, if they were to intercross, it would be indispensable that the male fertilising element should be transported by some means through the air; and the wind is the

[1] An extensive Jurassic flora has been described by Heer in *Mém. de l'Acad. Imp. des Sciences de St. Pétersbourg*, vii[e] série, tome xxii. No. 12, 1876.